in England and for the metropolis, only a few minutes before the occurrence of the terrible railway accident at Grantham on that night), I would be glad to know if they confirm the near approach to earth over the Brentwood Hills (as the end-point), and the position near Castor (as nearly the point of first appearance), assigned by this discussion to the meteor's flight; or if, as seen much sideways from its plane of fall, in counties north of Essex, the meteor's apparent line of flight may have been but little accordant perhaps, or even, quite possibly, not at all conformable with these sky-positions?

With regard to Mr. Denning's identification of the course of the above described bright meteor of the 13th inst., with a radiant-centre near *Honores*, I may mention in concluding that on the 19th inst., at 11.56, I noticed here a long-pathed streak leaving, 2nd mag. meteor shoot swiftly (about 30° in 1¼ sec.) from 195°, + 68°, in *Draco*, to 181°, + 39°, in *Canes Venatici*, leaving a thin white streak for 2½ seconds along its whole track. This course prolonged backwards about 60° proceeds from 350°, + 37°, only 3° or 4° from the place at 354°, + 39°, of the radiant No. 174 of Mr. Denning's list; and like the brighter shooting-star of June 13, it evidently belonged to this same June "Honorid" system. Mr. Denning's outline of the shower's duration, through the latter half of June and the first week of July, with a date of maximum on June 26, would thus seem, for this year's return of its meteor-period at least, to be in a fair way to be realised. A. S. Herschel.

Observatory House, Slough, June 20.

Purification of Sulphur.

It is never a very pleasant or gracious task to reply to criticism. When that criticism, however, is based on a misapprehension of the facts, and is consequently wrong and misleading, and is, moreover, enforced by whatever weight "authority" may carry, its correction is simplified down to a plain matter of duty. The criticism to which I refer is contained in Prof. Armstrong's address to the Chemical Society (*Chem. Soc. Journ.*, vol. xxxix. p. 1160), which has just come before me.

The passage to which I refer runs as follows. "To return to sulphur, an abstract account has recently been published in the *Proceedings* of the Royal Society (1894, lvi. 32) of observations by Threlfall, Brearley and Allen, on the electrical properties of pure (*sic*) sulphur, *i.e.* sulphur from the Chance recovery process purified by distillation and exhaustion in vacuo. Such a process cannot be accepted by any means as an exhaustive one, and it appears almost to be a case of 'love's labour lost' to apply to such material the infinite care which the authors appear to have taken in making the electrical measurements. Yet they arrive at the important conclusion that so long as a single modification be dealt with, such sulphur does not conduct while solid. A mixture of two modifications, however, does; but in view of the possibility of changes taking place during the production of the mixture, of conducting impurities being introduced or generated, it is difficult to regard this latter conclusion as established; the more so as the authors in question have found that, as the temperature was raised the conductivity of the sulphur increased slightly up to the melting point, when there was an enormous increase."

Prof. Armstrong prefaced the paragraph which I have quoted above, by some rather ungenerous remarks as to the supposed attitude of physicists towards the question of chemical purification of material. It is now eight years since I began to endeavour to purify materials, so that I can, at all events, agree with Prof. Armstrong on one point, viz. that it is much more difficult to purify a substance up to the furthest limit of chemical discrimination, than it is to determine its physical properties afterwards. In the passage to which I now allude—and which I do not intend to quote, for I feel sure that Prof. Armstrong will, on consideration, agree with me that it is better forgotten—the "tongue of the scorner" is thrust out at those physicists who take no thought as to the condition of material examined by them. However true this may once have been, I am sure that nowadays it is a mere superstition to suppose that physicists, as a body, are callous on the subject of purification. A great deal of modern work in this domain of chemistry has been done by professed physicists; and indeed, though I have myself known several physicists enormously interested in questions of purification, I have only known one chemist whose life-long endeavour was to get things pure, and that was the late Prof. Josiah P. Cook, of Harvard. Passing to the immediate point, Prof. Armstrong objects to the use of the word "pure," made by myself and my co-workers. With regard to this, I may say that Prof. Armstrong labours under the disadvantage of only having an abstract under his notice. In the paper (if it ever gets published) he will find that this very matter is discussed at, perhaps, too great a length, and the conclusion arrived at that the word "pure" ought to be kept for substances in such a degree of purity that existing chemical or physical means fail in discovering any foreign substance. "Pure," therefore, as we have used the word, has no meaning except in connection with the existing state of the art of chemistry, and was adopted by us rather than the word "purified," as the result of some consideration, in which a desire to avoid pedantry had some weight. Since Prof. Armstrong refers eulogistically to Stas—as who would not—I may perhaps refer him to the following passages by Stas himself (*Bulletin de l'Académie Royale des Sciences, &c., de Belgique.* 2 séries, vol. x. p. 253), than whom no one could be more careful as to the use he makes of the word "pure."

"Jusqu'ici, il n'y a que M. Dumas qui ait tenté de faire la synthése du sulfuré d'argent. Pour déterminer le rapport proportionnel de ses éléments, il a sulfuré directement l'argent par *du soufre pur* qu'il faisait passer en excès." . . . "En suivant cette méthode, j'ai fait deux séries d'expériences: la première, comprenant trois synthèses par *du soufre pur* amené en excès." Stas gives no details as to the preparation of his "soufre pur" (which he would certainly have done had elaborate precautions been taken); and as a matter of fact, Stas' sulphur was probably far less pure than mine, for in those days there was no "Chance" sulphur. So much for the word "pure."

Prof. Armstrong considers that the process of purification employed by us is "by no means an exhaustive one," &c. A process is, we take it, exhaustive when it exhausts the resources of physics, including chemistry. Now, we spent four years in trying all likely and many unlikely methods of purification. We finally, by sheer good fortune, received some Chance sulphur, and our methods of discrimination at once revealed to us that, when dust and water were removed, it was purer than any we had been able to prepare hitherto by the most elaborate means; and we adopted it as our source of sulphur in consequence of this discovery. On the other hand, there are doubtless degrees in the purity attained by the commercial product. The first lot we received (a present from Mr. Chance) was much purer than some afterwards purchased; and with this later sample the process we employed for purification would probably not have been sufficient.

With regard to the question of the adequacy of the purification—for this is the important point—we arrived at a stage at which no chemical means enabled us to detect any impurity whatever, and the specific resistance (if one may so misuse the term for a body which does not obey Ohm's law) rose above 10^{28} C.G.S. units; our limit of discrimination, and probably the furthest hitherto *certainly* attained.

As we had found that the purer the sulphur the better it insulated, as with this sulphur we could absolutely find no impurity at all, and as all our means of purification (except by repetition) were exhausted, we felt that we had done all that could possibly be done.

The conclusion at which we arrived—viz. that so-called mixtures of crystalline and amorphous sulphur conduct, whereas pure crystalline sulphur does not—seems to us to be of considerable importance, and we therefore spared no pains to assure ourselves of its truth. For this purpose we prepared crystalline sulphur films, ascertained their property by non-conductivity, and then converted them into the conducting mixture by appropriate heating and cooling. Conversely, we caused conducting "mixtures" to become non-conducting by annealing. Prof. Armstrong's criticism, therefore, is wrong, and the uncertainty, which he assumes, does not exist. It is as certain that crystalline sulphur is at least about a million times more non-conducting than "mixed" sulphur, as that copper conducts better than glass. We spent some years in assuring ourselves of the truth of this proposition, and we feel that we should be shirking our philosophical duty were we to allow this conclusion, so laboriously reached, to be set aside, or rendered nugatory, by a criticism based on a misapprehension, and enforced by all the weight which Prof. Armstrong's utterances so rightly carry.

I confess I am unable to reply to that part of Prof. Armstrong's criticism in which the above conclusion is supposed to

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be in some way dependent on the fact that any variety of sulphur increases in conductivity with rise of temperature. Unless it is suggested that we did not know one temperature from another, I fail to understand this criticism. Richard Threlfall.

University, Sydney, N.S.W.

I see no reason to recall what I have said regarding the general attitude of chemists and physicists on the question of the influence of minute traces of impurity; and when I come across the remark in Messrs. Threlfall, Brearley and Allen's paper in the *Phil. Trans.*, that "it is not too much to say that the electrical action of most bodies in a pure state is entirely unknown at present," I feel there is not much difference of opinion between us.

Then, as to my being guilty of that unpardonable crime—pedantry—it has always seemed to me that those of us who undertake scientific work should also strive to be scientific, *i.e.* exact, in their use of language. Those who had the great good fortune to be present a year or so ago at the Nature dinner, and to hear Huxley's marvellous speech—almost, if not the last he delivered—will recollect how strongly he insisted on the importance of greater care being taken in the writing of papers describing scientific inquiries. In a conversation I had with him afterwards, he greatly lamented the careless manner in which such work was too frequently done.

Now if *pure* mean "free from mixture," a pure substance must, as I have said, ever remain an ideal conception; the purist must ever regard all things as impure. Prof. Threlfall tells us that "the word pure has no significance except with respect to a definite state of the art of chemistry." I would rather accept the meaning which is to be found in the dictionary, *pace* Stas even; and would prefer to assert that the word too frequently has no significance except with reference to an indefinite state of the mind of the person—chemist or physicist—using it. To my mind, there can only be degrees of impurity—not of purity.

Whatever time Prof. Threlfall and his colleagues may have spent in seeking to purify sulphur, the fact remains that their experiments were made with sulphur which they obtained by chance, and that the only method of purifying it they adopted was to distil it several times in vacuo, after filtering it while molten through glass wool and platinum gauze, and then to fuse it in vacuo—in order, they tell us, to get rid of gases (probably water vapour, they say) given off even from the purest samples. But distillation in vacuo, even when followed by fusion in vacuo, can scarcely be regarded as a process which "exhausts the resources of physics, including chemistry."

"Chance" sulphur is prepared by burning sulphuretted hydrogen. It is probably impossible to burn sulphur without producing some sulphuric acid. Messrs. Threlfall, Brearley and Allen, however, do not even refer to the possibility of its presence, and apparently took no precaution whatever to eliminate it, if present.

They tell us that on breaking up such sulphur after it had been strained while molten through glass wool and platinum gauze, it emitted a horrible smell of gas-lime, "which shows that it requires to be distilled if sure results are to be obtained." I imagine, therefore, that the sulphur they used initially was by no means so remarkably "pure"; as they also state that gases were given off even from the purest samples when fused in vacuo after distillation, it may well be doubted whether so simple a process as mere fusion could suffice to effect the necessary final purification.

Prof. Threlfall's statement that conducting "mixtures" were caused to become non-conducting by annealing, is apparently a good answer to my criticism; but by no means finally disposes of it. The structure of the two materials may have been very different, and such in the one case as to allow an impurity to act, which in the other case might be inoperative. By my reference to the conductivity of sulphur at temperatures above its melting point, I meant to imply that the behaviour described afforded indication of the presence of impurity; for I do not believe that even molten sulphur is a conductor. Of course, at present, this is but an opinion, but it may not be inappropriate to direct attention to the recent most remarkable observations of Dewar and Fleming on bismuth, showing that an amount of impurity altogether beyond detection by chemical means may entirely alter electrical properties.

I still, therefore, regretfully retain my opinion, and fear that, notwithstanding the great care lavished on the work of Prof. Threlfall and his colleagues, it will be necessary to repeat it, perhaps over and over again—a possibility which they apparently themselves foresee in the introduction to their paper—before so remarkable a conclusion as that they have arrived at can be regarded as established.

H. E. Armstrong.

Increasing the Efficiency of Röntgen Ray Tubes.

Mr. J. C. Porter, in a letter in Nature of June 18, describes a method of increasing the efficiency of a Crookes' tube. I have for some weeks used another very simple method to obtain the same result. This consists in placing the flame of a small glass spirit-lamp in the angle formed by the Crookes' tube and the wire passing to the kathode, and allowing a series of small sparks to pass to the flame from the wire.

Burnley, June 29. T. G. Crump.

THE POSITION OF SCIENCE AT OXFORD.

WHILST the study of natural science has been progressing rapidly in other universities and colleges during the last ten or fifteen years, it is a matter of common knowledge that it has progressed very slowly indeed in the University of Oxford. It would be incorrect to say that it has not progressed, for there has been during the last few years a steady, though very gradual, increase in the numbers of men reading for honours in the final school of natural science. In 1885 twenty-two men obtained honours in sciencce, in 1895 there were forty-three names in the class list, and a rather larger number in 1894. The school has just doubled itself in ten years, but for all that the numbers are still small, and out of all proportion to the provision that has long existed for science teaching in the University. It must be understood at the outset that the University, considered as a body separate from the colleges which compose it, has not dealt ungenerously with science. The staff of professors, and the emoluments attached to their chairs, compare favourably with those of any other university in Great Britain; and Oxford actually set the example, at great cost to itself, of building a museum and equipping laboratories for educational purposes. Moreover, the opportunities of scientific study in Oxford are greatly enhanced by the existence within the precincts of the museum of a first-rate scientific library, such as is not possessed by any other college or university in the kingdom. It is a strange thing that when it has so many advantages, Oxford has allowed itself to be completely outstripped in this particular path of intellectual progress.

It is the purpose of the present article to discuss the possible causes of comparative failure of the science school at Oxford. A complete failure it is not, for, however poor its numerical results may be, it has long been recognised that the attainments of the limited number of scientific men which it turns out compare well with those of men who have been educated in other places.

It is commonly supposed that the prime cause of the insignificant numerical result is the small encouragement given to scientific study in the shape of fellowships and scholarships; and those who hold this opinion believe that if the colleges were to do what is conceived to be their duty in this respect, the science school would progress by leaps and bounds.

With respect to scholarships and exhibitions, it is apparent, from an analysis of the figures, that science does not get what may rightly be held to be its due. The *University Calendar* for 1896 shows that there are in Oxford some 500 scholarships of an annual value of £80 a-piece, and in addition some 225 exhibitions, the annual value of each of which may be placed at £40, These figures apply only to college scholarships and exhibitions, and so it appears that the colleges, apart

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